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The potentials and limitations of rational choice theory: an interview with Gary Becker

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Gary S. Becker (Pennsylvania, 1930) is a university professor at the Departments of Economics, Sociology, and the Graduate School of Business at the University of Chicago, Illinois. Becker earned his undergraduate degree from Princeton University and was awarded a PhD by the University of Chicago in 1955 for a thesis on the economics of discrimination, under the supervision of Milton Friedman. After teaching at Columbia University from 1957 to 1969, he returned to the University of Chicago where he has been based ever since.

Becker's work and research interests encompass a wide range of topics, unified by what he calls *The economic approach to human behavior* (Becker 1976). He considers this refined version of the neoclassical theory of consumer behavior as a method that can be applied to analyzing individual choices beyond the boundaries of traditional economics domains, including discrimination, education (human capital), crime, addiction, the family (marriage, divorce, fertility), and altruism. Becker's path-breaking work has been recognized with numerous honors, including the John Bates Clark Medal (1967), and the Presidential Medal of Freedom (2007). In 1992, he was awarded the Nobel Memorial Prize in Economic Sciences "for having extended the domain of microeconomic analysis to a wide range of human behavior and interaction, including nonmarket behavior" (Nobel Prize press release).

Professor Becker was interviewed by Catherine Herfeld at his office on the Campus of the University of Chicago on December 8th, 2010. The discussion ranged over a number of issues including the consequences of the recent financial crisis for the economics profession, the role of mathematics in economic modeling and the role of modeling in economics, the significance of the rationality-principle, and the

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development of Becker's 'economic approach' and its distinctiveness from behavioral economics.

CATHERINE HERFELD: *Professor Becker, looking back on the recent economic crisis and the failure of the majority of economists to forecast what happened, do you think the economics profession faces a crisis?*

GARY BECKER: No, I do not think so. I think that the profession will be affected in the sense that people will be working on problems in order to understand the financial crisis—and people are doing this already. But forecasting major events like that is very hard to do in any field. And I think to hold that as a standard of what one can do in a field is not the right standard. It is true that specialists, maybe in fields such as 'asset pricing' and 'assets and banking', should have seen that banks had high ratios of assets to capital and that only a few people in the profession forecasted that that might be a problem, some of them maybe even only by accident. But I do not think that we can observe a crisis in economics.

What we can indeed observe is much more a tension, particularly in the macroeconomic literature, in trying to understand what actually happened. Already before the crisis, there was a literature on the financial sectors that was concerned to understand business cycles by making use of the so-called real business cycle theories. Those theories are on the defensive now. I think, as we go forward, theories of business cycles will have to give much more attention to the financial sector. In that sense the crisis has taught economists an important lesson, but it will not radically change what most economists are doing.

Following the crisis, many economists and methodologists have argued that more realistic behavioral underpinnings of economic theory would have made forecasts more accurate. Do you think that one of the things the recent crisis has shown us is that people just do not behave rationally? Or did the crisis rather show exactly the opposite—that people did in fact react to incentives and that the consequences of introducing new financial instruments were just not foreseeable?

I think it is mainly the latter. There were incentives, both on the borrower and on the lender side, that these subprime loans would be

made available at the lowest interest rates; and there was pressure from the government to do so; and probably those involved did not understand the financial instruments. Now, is it that we have to change our theories radically with respect to their behavioral structure or even switch to a new behavioral framework? There is very little evidence that would support such a move.

There is a whole field of behavioral economics that I follow pretty closely, and parts of it I have even contributed to. But did the behavioral economists predict the crisis any better? When taking a look at the literature, one does not find better results. The rational choice model is an abstraction and as is the case with all abstractions and all theories from whatever discipline, say physics, you abstract from some things that sometimes may be important. And this is also true of the rational choice model. In terms of understanding the crisis, I do not think that more realistic behavioral assumptions would solve the problem. It has always been difficult in rational choice models to adequately account for the coordination of people's expectations. To some extent, the crisis involved the coordination of irrational expectations. This might be something we should think about and improve.

With respect to how the crisis affects our models in terms of being based on a more realistic assumption structure: what will occur is that models become refined to help us understand what happened. But I do not see a fundamental change in the models with respect to the underlying structure of human behavior, nor do I see a need for such a change.

So was the crisis more a source for a critique against the rational expectations hypothesis, rather than towards the behavioral core of economic theory, i.e., rational choice theory?

Well, to some extent it was a critique of that hypothesis. The expectations turned out to be, to some extent, not rational; there is no question about that. Price increases were for example expected to continue. But the theory of rational expectations always said that people make a forecast and could coordinate on a bad forecast. That has always been part of the theory, however there is more attention being paid to that phenomenon now.

In the post war period, mathematization was (and still is) considered a prime virtue of economic theory, and important to improving the

scientific status of economics. Finance, especially, embraced highly technical models that produced precise calculations and predictions. However, it is claimed that it is exactly this extensive use of mathematical models that ultimately weakens the scientific status of the discipline; the failure of economists to predict the crisis being taken as evidence. What role should mathematics play in economics? And in which ways and to what extent can these mathematical models inform us about the complexity of the social world and of the economy?

Let me give you an example. The great depression was a far more serious crisis than this financial crisis we are currently facing. Economists made no use of mathematical models then. Did they predict that crisis very well? No, they did not. Going back and analyzing this failure is a good lesson to take. The economists back then did just as badly as the economists do now in terms of predicting. So, I do not think that the problem lies in the use of mathematics.

There is a lot of critique against mathematics in economics, from non-economists, from Austrian economists and from other groups, and I think it is misplaced. Mathematics can be a very useful servant; when it becomes the master, we are not in a good situation. However, I do not think it has become a master in economics. I think we made mistakes in understanding how economies move forward, even in understanding the pricing of derivatives. But one can make these mistakes, and plenty of mistakes have been made, without using any mathematics. Sociologists make a lot of mistakes without using mathematics. So I do not think that the problem is the use of mathematics per se.

The discipline will continue to be heavily mathematical but hopefully will learn from this crisis. I always say that mathematics is useful but you have to have good economic content. If you do not have good economic content then, whether you do it mathematically, verbally, or with a graph, you are doing bad analysis. I am not one of the most mathematical of the economists; a lot of people use much more mathematics than I do. But I have never thought that the use of mathematics is the problem. Only bad use of mathematics is a problem and will continue to be a problem. If we did it all verbally, would that improve our science? Economics was a verbal science until the 1940s and I would say we are now doing much better than the economists back then.

How do you think economics should be done?

The way I like to put it is that we have to have a dialogue between the theory or model and the data. Theory informs us about what data to look at and how to interpret the data. But data also informs the theory. So if you have theoretical predictions that continue to turn out wrong, you have to change the theory. As I said, the real business cycle theory ignored the financial sector. This crisis showed us that the financial sector is really important. Economists who are working in that area are going to change that now. And that is how I think it should be.

A discipline where the theory is isolated from what is going on in the world will become a sterile discipline. And a discipline that is only looking empirically without any modeling will also become sterile. I think the disciplines that are active, that are productive, are those that have an active dialogue between the two aspects. That does not mean that everybody has to be doing both, but I have always believed that the ideal economist works with theory, looks at the data, gets a data feedback on the theory and vice versa. Some people just work on theoretical issues and that is fine. Some people just put data together in a useful way, which is fine too. But the bulk of them should be looking at this combination.

Coming to your own work, you had a major influence on 20th century economics by introducing a broad range of human motives into economic theory, something that could be considered as going into the same direction as what is today known as behavioral economics. Yet, unlike behavioral economists, you retained “an irrational passion for dispassionate rationality”, as you once expressed it.¹ Why did you decide to stick with the rationality-principle in explaining and predicting human behavior?

I felt that the rationality-principle was a powerful tool that was useful for explaining behavior. In all my work, even if it is purely theoretical, I am looking at data, talking about observations, sometimes even gathering data. For example, when I used a rational model of altruism to look at the family, it seemed to help me to understand why parents do support their kids and under which conditions they do so, to analyze relations between spouses, and so on (e.g., Becker 1981). When I went to crime, I thought it helped us to understand crime and deterrence and

¹ Oral remark at a luncheon seminar at Tulane University in 1986 (Choi 1993, 19 fn.).

the effects of education (e.g., Becker 1968). In areas where the rational choice model does not work so well, one has to modify it, but I have been persuaded, at least by my own thinking and by looking at the world and the actual data, that it does a very good job, and that there is no other comparable approach in the social sciences with the same degree of explanatory power, or even anywhere near.

And you can look at a lot of behavior, not only to bring in aspects such as altruism or envy, which are now part of behavioral economics, but also to include the idea that people discount the future hyperbolically instead of exponentially. David Laibson, who introduced the idea of hyperbolic discounting into economics, makes use of axiomatization (see, for example, Laibson 1997). Everything else he uses is very much the same as what I use. Maybe people are hyperbolic; if that is true, we have to alter our theories, but that is where we would need feedback from the actual data. Yet we still have a basic framework in use, which is some version of the rational choice model. If you would start to abandon this framework, you would end up with a loose group of findings. If you want to abandon rational choice theory altogether, you have to substitute it with a new framework, and I do not see any new framework available at the moment—neither in the behavioral economics literature nor anywhere else—that has comparable explanatory and predictive power. That is the test.

It is an old saying that you need a theory to beat a theory. That does not mean that you cannot extend the existing theory or modify it—you can and you should. As we learn more, we will modify rational choice theory. Maybe fifty years from now it will not be like rational choice theory anymore, because by then it will have been modified and changed in so many ways. That is how things evolve. Einstein modified Newtonian mechanics, but Newtonian mechanics is still applied to a wide range of phenomena.

Would you consider behavioral economics to be a revolution in economics analogous to the transition from Newtonian mechanics to Einsteinian relativity theory, in that, although the rationality-principle has been questioned fundamentally, the rational choice model remains in use in economics as a parallel framework for explaining a wide range of behavior?

Well, of course I would not say that behavioral economics has been as important as Einstein's revolution; I would not compare them with one

another. In fact, I do not think that behavioral economics is a revolution. However, it has added some insights into human behavior and those insights, to the extent that they are verifiable, will be absorbed into the rational choice model. They will not lead to a radical change of the model. The real issues are how important are those insights and where do they apply?

So, for example, the explanation that consumers were somehow misled in the credit market, and that this in turn contributed to the financial crisis: I think there is very little empirical support for that. A lot of consumers were making pretty rational decisions, even those who were taking out mortgages with low interest rates and low down payments. Maybe they were going to default. But they did not default on their own capital. They defaulted on the lender's capital. So I see very little evidence from this that consumers are not rational, in the sense that the rational choice model cannot explain most of what they did.

How would you assess the epistemic value of these recent developments, such as behavioral and experimental economics, in terms of providing us with new knowledge about how the economy works?

I think they have been stimulating in terms of leading to further lab and field experiments. They have shown that people can be fooled by how a question is framed. And they have shown that similar people in different contexts may behave differently. I think that all this is valuable. But at the bottom line, economists deal with markets and group responses, and there it is hard to see major modifications coming out of behavioral economics as yet.

But has there not been a shift in the last eighty years towards economics becoming concerned with explaining individual choices rather than aggregate behavior?

Well there was a shift in putting much more reliance on individual choices in terms of modeling behavior. But when you look at the data economists have mainly used, the ultimate goal of most of the studies is to look at how people respond to incentives. Economists can make use of individual data panels and other data based on observations about the individual. However, what we are interested in are aggregates and market relations. For example, if the political aim is to subsidize education, economists do not care about how you respond or I respond

in particular. Maybe there are differences in how Germans respond or Americans respond, or how people who study at the University of Chicago respond in comparison to how students from Columbia University respond, and I guess that we would care about that. But not about how the individual responds. In my opinion, this is a fundamental difference between psychology and economics.

*During the 1970s and 1980s various psychologists, such as Herbert Simon, Daniel Kahneman, and Amos Tversky, showed that people systematically violate the rationality principle and argued that economics could be improved by making the underlying psychological assumptions of economic theory more realistic. How seriously did you take these new developments at the time? Did they influence you or did they make you question your own approach? Did they inspire your later work—for example on endogenous preferences in your *Accounting for tastes* (Becker 1996)?*

Well, it is hard to know where the influences come from. How I proceed in my work is that I try to keep up with what is being researched on in the discipline and then I think about potential contributions. Kahneman and Tversky made contributions that were very influential and highly cited in economics. So, for example, looking at the utility function as hinging around some usual position—that there is a reference point and you are highly risk averse towards losses, and so forth. I do not think it is completely clear from market evidence that its effects are significant, but I do believe the reference point analysis is itself important and maybe some of that literature influenced the work I did with Luis Rayo, very formal work on evolutionary theory where we show how to derive reference points and some other properties (Rayo and Becker 2007).

Their work has been important to psychologists and it has had some influence on the economics profession. And I would say it has had some influence on my work, although it is hard for me to know exactly how much. But I like to believe that my work has evolved and that what I believe today is not the same as what I believed in the 1980s and 1970s. I learn from what other people are doing; that is what intellectual interaction does for you. So I do not think their work has radically changed my approach, but I have been affected by it.

Turning to a somewhat different issue: rationality is a concept that originated in philosophy and its various economic formulations and

uses have been discussed extensively in the philosophical literature on the methodology of economics, such as by Alexander Rosenberg, Philip Mirowski, D. Wade Hands, and Mark Blaug. Were you ever interested in that literature? Or where did you get inspiration from when thinking about improving how rationality is conceived of in economics?

Primarily, I get inspiration from my own discipline, economics. For example, I wrote my doctoral dissertation on racial discrimination.² I wrote three papers before I did my work on discrimination. One was with Milton Friedman on Keynesian models (Friedman and Becker 1957), one was on monetary trade (Becker and Baumol 1952), and one was on international trade (Becker 1952). I would say that the last two were rather traditional papers; the one with Friedman was not traditional as we were very critical of Keynesians, but I got a lot of that from Friedman, so it was more him than me.

The work on discrimination I would say was my own work. As an undergraduate, I always felt that economics was too narrow. I thought of being a sociologist, but I found sociology difficult and so I was not satisfied with it. Friedman really taught me—although his own way did not take that path—that economics could be a powerful tool, and I began to think about racial discrimination and how economists were not discussing such an important topic that affects so many people. That is how I got into it. It was not from the methodological literature. I read some of that literature. I read Karl Popper and I studied Rudolf Carnap when I was a graduate student here at the University of Chicago, so I did read a lot of philosophical literature that was relevant to economics. But I cannot say that it directed me towards the topics I dealt with.

Friedman's article on methodology of economics (Friedman 1953) was very important—that I did read very carefully. I knew that article very well and it influenced me to a certain extent. Maybe not on the topics that I chose, but more in how I approached these topics. That value theory does not have to be realistic in any dimension was the part influenced by Friedman, and he got it from people like Karl Popper and others.

² Later published under the title *The economics of discrimination* (Becker 1957).

In an interview, the economist Leonard Rapping said that “many Chicago people would argue that the world is in fact competitive. They tend to believe their own pragmatic myth” (Klamer 1984, 221), i.e., that people in fact maximize profits and utility. In your own work, you have often denied a commitment to a realist interpretation of the rationality-principle. In your Nobel Lecture for example you state that the economic approach is a “method of analysis, not an assumption about particular motivations” (Becker 1993, 385). However, to make your approach work, the rationality-principle seems to figure as a behavioral assumption. It has to be at least approximately true to provide meaningful explanations and predictions; one cannot derive a true conclusion from false premises. Could you comment on this seeming contradiction? Does this for example reflect the influence on your work of the strong version of instrumentalism propagated by Milton Friedman?

The way I restated Milton Friedman’s view in my own thinking is that one cannot evaluate a set of assumptions individually. You have to evaluate the whole set of assumptions collectively, because that is what a model is: a collective set of assumptions about behavior that is predicting behavior. And how do you evaluate a collective set of assumptions? It is very difficult to say “this assumption does not work” a priori, because it is the collective set what is relevant. The only way to evaluate assumptions is to ask whether this collective set of assumptions is in fact explaining behavior. Are you doing well in predicting and understanding how people respond to a tax cut, tariffs, globalization, returns to education, and the like? So that is my methodology.

Now, it is true that I like to believe that the individual assumptions are in some sense reasonable, but you have to look at them together. And I think that this is a problem with the behavioral economists. They take an assumption, for example that people cannot calculate probabilities very well and that there are other people on the other side who will try to exploit that weakness in them and will offer them various deals. If there is competition on the other side of the market that will mean that they will be offered some compensation. That is what competition does. For example if we play any kind of gamble, let us say we throw a dice and you think that the most likely outcome is ten, well I can exploit that. I do not even have to cheat, let us play. But if a lot of people are going to want to exploit you, we are going

to have to compensate you. So you have to ask what the market equilibrium looks like. You are a fool, but the market is competing to take advantage of that. That is how I would analyze that problem, and that has been one of my critiques of some of the behavioral economists: they do not embed their insights in a complete model of behavior.

Are you thereby implying that equilibrium analysis is a worthwhile undertaking?

You do not need to use the concept of a complete equilibrium. You can do it with modified equilibrium: as long as there are other people who are recognizing that I am a fool, they would compete. If you were the only person with that information, you could exploit it. A monopolist for example could exploit me. One of the great advantages of competition is that it prevents such exploitation, and you do not need perfect competition to have a strong effect in that direction.

So how does equilibrium analysis feature in your 'economic approach'?

Economists from the Austrian school hate equilibrium analysis in some sense, but I never understood their criticism. What do philosophers not like about equilibrium analysis?

Philosophers raise several objections against this way of analyzing the economy, one being that the application of the concept of equilibrium to an environment which is actually never in equilibrium is meaningless and does not provide us with any understanding about the real world. Take, for example, comparative statics.

But what do they substitute for it?

Well, philosophers do take a critical perspective; they often tend to evaluate the shortcomings of a theoretical framework first.

Yes, but as I said before, you need a theory to beat a theory. I think the equilibrium concept in economics is very subtle. It could take into account and does often take into account dynamic issues, changes—it is not static and it is not stationary. You have dynamic models of behavior that incorporate the concept, so they are still equilibrium models but it is dynamic equilibrium. It is a broad issue.

I have read some of the literature on the critique of equilibrium, not so much by philosophers but by the Austrian school of economics, and I could just never make sense out of it, because I do not see what they are substituting for it. Even Friedrich Hayek, who is listed as one of the top Austrians, if you read his analysis, you see that he is using equilibrium analysis.

But Hayek suggested the concept of a ‘reflective equilibrium’, which has however so far not been formalized.

Well, dynamic equilibriums can be formalized, as dynamic general equilibrium analysis does. I agree with you that a lot of analysis needs to be dynamic and comparative statics is not the right analysis for every issue. In economics, we are of course trying to improve, but I think we can do so with the tools we have available. I do not think that there is in principle any philosophical barrier to doing so. We could do it with a rational choice model or any other model. I do not think that would destroy the concept of equilibrium, and I do not think we should try to destroy it, because I think it is a very valuable concept.

In your work, you mainly look at aggregate demand and supply curves. In your textbook *Economic theory* (1971), for example, you look at a model of the irrational behavior of households to show that “the basic demand relations are derived fundamentally from scarcity alone rather than from an assumption that behavior is ‘rational’ and that the main conclusions of demand analysis [i.e., negatively inclined market demand curves] stem from a much more general principle than rational behavior—the scarcity of resources that defines an economic problem. Accordingly, we are able to derive the usual demand functions even when households behave ‘irrationally’” (Becker 1971, 11 fn.). Yet, you go on to use the concept of rationality because of the power of the implication “consumers prefer more to less” (which is empirically questionable) and you say that a model that implies such behavior is to be preferred. Why is that to be preferred? And why exactly do you need rationality if scarcity is what is fundamental? Or, to put it differently, do you think that the rationality-principle is a necessary ingredient of economic theory and, if so, why?

Do you think it is empirically questionable that people prefer more to less? I do not think so. I do not see people giving away, except to charity

but that is another good we would introduce. Yet, looking at the world, do you see many people who prefer less to more?

Well people become satiated or even reject striving for endless material improvement.

Are people satiated with regard to leisure? Anyway, let us get back to your question. I argued that the rationality assumption is required to introduce the aspect that people prefer more to less, which in turn helps us to understand market outcomes and explain prices. In the analysis you cited I assumed that prices are given to consumers and producers, but I think you need to have some analysis to answer the question, where do prices come from? Maybe there are other theories you could use but you need to amend or add to the probabilistic type models some theoretical framework that tells you what types of prices are finally picked out. And for that, rational choice analysis really is very useful.

Are you after truth?

Absolutely! I think there is a truth out there. We are only approximating it but we are getting better. I think that the goal is to find the truth and I think there is something like that. I know that there is a lot of philosophical discussion about what truth is, which I however do not really find useful. I think there is a truth, and I think that economists have found a significant amount of the truth in economic behavior. There are a lot of things we do not know, but there are also a lot of things we do know, which the non-economist gets completely wrong. A simple idea like showing that when gasoline is substituted, people are going to buy more gasoline elicits some truth about people's behavior. And these are important truths. This is what I call the truth in a particular case and this is what I want to find out and analyze. So, yes, I am after the truth.

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